THURSDAY, MAY 23, 1907.

MENDELISM.

Mendelism. By R. C. Punnett. Pp. vii+84. (Cambridge: Macmillan and Bowes. London: Macmillan and Co., Ltd., 1907.) Price 2*s.* net.

A SECOND edition of Mr. Punnett's "Mendelism" has followed the first after an interval of two years. The book gives a very good account of Mendelian work. Issuing as it does from Cambridge, the source of by far the greater part of the Mendelian discoveries of the last six years, it is the most authoritative account of this subject, and as it is eminently readable it is the very book for anyone who wants to know what Mendelism is. It is cheap and of a very convenient size, and we cannot suppress an expression of our admiration for the beautiful purple colour of the cover of the second edition.

At the same time, no good can be done by refusing to face the fact that the truth of the Mendelian doctrine is not universally admitted. No one denies the extraordinary interest of these discoveries. He must be a very callous man who is not fascinated by the way in which the proportions 9 : 3 : 4 and 9 : 7 and the phenomenon of reversion in certain cases have all been brought into line. But we hold that he must be a very rash man who accepts without further question the doctrine of gametic purity. Yet it is just in the sphere of interpretation that Mendelians are so certain. Once in this sphere, we can no longer be guided by facts—if we were dealing with facts we should be in the sphere of discovery—but by "such things as our mind conceives." And one's attitude should be one of continual, unceasing, and active distrust of oneself. The attitude of the Mendelian is different from this. He may reply that he is only triumphant about his discoveries; but we must remember that there is no fixed criterion by which we can say where discovery ends and interpretation begins; and we must be careful not to beg the question by defining discovery as that about which there can be no doubt.

There are those who deny the theory that the germ cells of an extracted recessive are pure in respect of the character of the organism which contains them, and who assert that the characters of the hybrid which produced it are not absent from, but latent in, those germ cells. If this were discovered to be the case, it would be regarded as a demonstration of the falsity of the doctrine of gametic purity by everyone who was not a Mendelian. But we should strongly condemn the proclamation of such a conclusion, because we think it is high time that the spirit which derives satisfaction from the victory of one opinion over another should be swept from science. There is no place for the party system in science; because it tends to make the triumph of truth the main object and truth itself a secondary one. We are not arguing that Mendelian theory is untrue, but that the attitude of anyone daring to say of anything "this is true" should be apologetic rather than victorious.

There is another and a larger point of view from which we may examine the Mendelian position: it is that which refers to the relation between the Mendelian and the material with which he deals. The differences between biometrician and Mendelian have been due partly to the fact that these two sets of workers have dealt with different sets of facts. But we are concerned with the difference between their attitudes to the same classes of facts, and with the paradox that in spite of this difference they both claim to have introduced exact methods into biology. How is it that the two schools which claim to have introduced the exact method into the study of biology are not at peace? What is the relation between the methods of the two schools? One author attempts to express the difference in the statement that the Mendelian deals with units and the biometrician with masses, and illustrates this view by saying that the difference between the relation of the biometrician and that of the Mendelian to the units with which they deal is the same as the difference between the relation of the physicist and that of the Maxwellian demon to the units (the atoms) with which they deal. The physicist and the biometrician deal with them in masses. The demon and the Mendelian deal with them separately. It is striking testimony to the callousness of biologists to general discussion that it has never been pointed out that this comparison, though plausible, is based on a fallacy. To anyone who tries to take a broad view of the matter, the truth or falsity of the statement (repeatedly made by Mendelians) that the biometrician deals with masses while the Mendelian deals with units is the most interesting question presented by this whole subject. For if the Mendelian really does deal with units while the biometrician deals with masses composed of these units, and if the Mendelian sets out with the object of enabling himself to predict what will be the result of a given union, and succeeds, while the biometrician starts on the assumption that a knowledge of the ancestry of a given pair does not enable him to predict the character of its offspring, there is little to be said for the "application of exact statistical methods to the problems of biology."

But is it really true that the Mendelian deals with the units of which the biometrician's masses are composed? We believe not. In order to see what the real state of affairs is we must try to begin at the beginning. The difference between the two schools lies in the difference between their respective attitudes to natural phenomena. The biometrician says, "We look at them as close as we can and we see nothing uniform." The Mendelian, "We look as close as we choose and we see everything uniform." The latter does not pretend that "dwarf" peas are not variable, but treats them as if they were all the same. The former does not pretend they are not all "dwarf," but treats them as if they were all different.

The exactness of the biometrician makes him count the number of hairs per square centimetre on the lower leaf surface of *Lychnis vespertina*; the exactness of the Mendelian enables him to tell at a glance in a row of hybrid stocks which are hoary

and which are glabrous. The two forms of exactness correspond to the two ways in which we may try to make certain of hitting the bull's-eye of a target when we shoot at it. We may either improve our marksmanship or enlarge the bull's-eye. The latter is the only method of ensuring uniformity, of enabling oneself to predict the result with certainty. To this the biometrician justly replies, "This is no real uniformity. It is an ideal uniformity substituted for a real variability. Your shots are scattered round the centre of your bull's-eye just as mine are scattered outside mine. I never hit. My bull's-eye is a point. I keep a record of the deviation of every single shot from it. I am faithful." To which the Mendelian replies, "I always hit. I keep no such records. I am successful." We do not hold a brief for either party. A bull's-eye so large that it cannot be missed is as unfair as one so small that it cannot be seen is unpractical. All we wish to insist on is that because Mendelians can predict and biometricians cannot, it does not follow that the units with which the Mendelian deals are the units of which the biometrician's masses are composed. The Mendelian's units *are* the biometrician's masses, except when the latter exceeds his limits and includes within his masses more than one such unit. The Mendelian can no more predict about the units of which the biometrician's masses are composed than the biometrician can, except when the biometrician includes more than one Mendelian unit in his mass.

CERTAIN ASPECTS OF SCIENTIFIC WORK.

Progress of Science in the Century. By Prof. J. Arthur Thomson. Pp. x + 536. (London: W. and R. Chambers, Ltd., 1906.) Price 5*s.* net.

IN a book bearing the present title it is surely unfortunate to find that progress in one branch of science, and that certainly not the least important, is wholly ignored. Yet while chemistry, physics, astronomy, geology, physiology, psychology, and even sociology each has a separate chapter devoted to it, not a word is said about the remarkable developments that have taken place in mathematical science during the century. The changes which recent times have witnessed in regard to our conceptions of the notion of space are certainly no less remarkable, and are quite as capable of being outlined in a popular work as the kinetic theory of gases or developments of theories of the ether.

The study of matter and energy is so closely connected with the study of space that a discussion of the former without some reference to the latter must give a reader an incorrect impression of the present state of physical science. But the omission of frequent and explicit mention of the work of the mathematician in certain other directions is also likely to be misleading. Why, the reader may ask, is Lord Kelvin's vortex atom theory recognised—we will not say accepted—by the scientific world while Mr. Horatio Gubbins (to use a fancy name) has been pestering secretaries of societies and editors in vain with his theories of gravitation or the ether, and no scientific man will have anything to say to him? It may be that the reader in question is Mr. Gubbins himself. If he studies the chapters on "The Scientific Mood" and "The Unity of Science," he will find in them every justification for believing that his grand discovery marks a new era in the advancement of science. If, again, he turns to p. 178 and reads the paragraph "Value of these Hypotheses" at the end of the chapter on physics, he will find the sentence:—

"These molecular and ethereal hypotheses are human imaginings—and nothing more; they are constructed in terms of one sense; that of sight; they are attempts to see that which is invisible, to invent a machinery of Nature, since the real mechanism is beyond our ken; but it must be observed that these hypotheses are not *vain* imaginings, for they prove themselves yearly most effective tools of research, and that they are not *random* guesses, for they are constructed in harmony with known facts."

This statement may be true enough, but the *suppressio veri* in the omission of all reference to the rigid framework of mathematical equations and formulæ supporting the hypotheses conveys a dangerous *suggestio falsi* to the unmathematical reader. Mr. Gubbins is perfectly convinced that his own theory, at any rate, is constructed in harmony with known facts, whatever may be said about Lord Kelvin's theories, which he not unfrequently has "*proved convincingly*" are wrong, and he may even take unto himself to say that he has at last discovered a theory which is something more than a mere human imagining. No book of the present kind should be issued which does not strongly emphasise the fact that the true test of every scientific theory is in all cases a quantitative test based on a comparison of the formulæ of the mathematician with the measurements of the experimenter. Otherwise the English reader will be led to believe that the needs of science, which are now being pressed forward, can be adequately met by the erection of laboratories and the endowment of scholarships for passing elementary examinations, while the brain workers who interest themselves in researches carried out in their own studies with ink and paper will find themselves, as time goes on, more and more unable to cope with the accumulation of unsolved problems that is being pressed on them from every quarter.

Descending to matters of detail, we find many important theories conspicuous by their absence. We need only specify the phase rule and the second law of thermodynamics as instances in point. Yet the very possibility of a world existing which is inhabited by living beings, including man, depends essentially on this neglected second law. It seems almost unnecessary, in view of this omission, that the author should apologise in his preface for the absence of any reference to radium on the ground that the book was printed before the discovery had been made.

It cannot be denied that in attempting to trace the scientific progress of a century, even in its barest outlines, in a volume of this size the author undertook an impossible task. It is probable that he would have done better if he had confined his attention to